of boilers and engines, when it is very doubtful if even the present story can be wholly utilized for a long time to come; rows of boilers and furnaces were erected some two or three years ago to supply steam to drive dynamos which are not yet made; tens of thousands of pounds have been expended on machinery to be employed in constructing two ten-thousand horse-power dynamos, and the armature of one of them, 43 feet in diameter, has had to be left abandoned only half finished, because there is neither money nor present need for such a dynamo at Deptford.

And while all these provisions for the future electric lighting of London on a vast scale were slowly proceeding, the present customers were left sometimes for hours, sometimes for days, and occasionally even for weeks in darkness: what wonder is it, then, that all over London there have have been growing up central stations supplying a direct current at low pressure, and that many of the householders who formerly received current from the overhead wires of the London Electric Supply Corporation have had their houses connected instead with the low-pressure underground mains of other companies?

To the world at large, however, the Deptford undertaking has been of immense value, for it has shown the possibility of practically using the very high potential differences absolutely necessary for economically transmitting power over such distances as that between Lauffen and Frankfort. Hence, maintaining 20,000 volts between *bare* wires running for 109 miles along the side of the Neckar railway, at a height of only 16 feet from the ground, sounds much less startling now than did Mr. Ferranti's proposal made and acted on five years ago to bring only one-tenth of this pressure, by means of india-rubber covered conductors, into locked transformer rooms built of brick in the basement of the houses supplied with current from the Grosvenor Gallery.

In fact, the results that have been attained through Mr. Ferranti's undaunted courage, and the well-filled purses of his friends, have led people to look on a pressure of 20,000 volts as they regard a velocity of 70 miles an hour, so that to day, in order to prevent boys climbing up any one of the 3000 ordinary telegraph poles which carry the wires from Lauffen to Frankfort, it is thought sufficient to merely paint a skull and cross-bones on every post as an indication of the deadly fate that awaits the climber.[1]

(*To be continued.*)

ON VAN DER WAALS'S TREATMENT OF LAPLACE'S PRESSURE IN THE VIRIAL EQUATION: IN ANSWER TO LORD RAYLEIGH.

My dear Lord Rayleigh,—As you are aware, I did not see your letter of September 7 (Nature, 24/9/91) till a fortnight after its date; and my reply has been further delayed for a week in consequence of the closing of Edinburgh University Library at this season. Even now I can refer only to the German version of Van der Waals's pamphlet.

Partly on account of its unfamiliar language, but more especially on account of a very definite unfavourable opinion expressed by Clerk-Maxwell (Nature, 15/10/74) I did not attempt to read the pamphlet when it appeared; and it was not till 1888 that, in consequence of some hints from Dr. H. Du Bois, I hastily perused it in its German form.

The passage which you quote from my paper (where, by the way, the printers have unfortunately put *resistance* for *resilience*) is certainly not a very accurate description of Van der Waals's method, but it represents faithfully the difficulties which I felt on first reading the pamphlet. I said that Van der Waals's "justification of the introduction of the term a/v^2 into an account already closed, as it were, escapes me." And I am not surprised that it did so. For the statement of Clerk-Maxwell had prepared me to look for error; and when, at the end of Chap. VI., I met with the formula

$$p(v-b) = R(1+at),$$

which, a couple of pages later (nothing but general reasoning intervening), somehow developed itself into

$$\left(p+\frac{a}{v^2}\right)(v-b) = R(1+at),$$

I naturally concluded that this was the matter adverted to. I spoke of the first of these equations as a "closed account," because of the process by which b had been introduced. To this point I must presently recur.

I had not examined with any particular care the opening chapters, to which your letter chiefly refers; probably having supposed them to contain nothing beyond a statement and proof of the Virial Theorem (with which I was already familiar) along with a reproduction of a good deal of Laplace's work.

Of course your account of this earlier part of the pamphlet (which I have now, for the first time, read with care) is correct. But I do not see that any part of my statements (with perhaps the single exception of the now italicized word in the phrase "the *whole* procedure is erroneous") is invalidated by it. No doubt, the sudden appearance of a/v^2 in the formula above quoted is, to some extent at least, accounted for; but is the term correctly introduced?

The formula you give would lead, on Van der Waals's principles as to the interpretation of $\frac{1}{3}\Sigma(mV^2)$, to

$$v(p+K) = R(1+at),$$

or

$$v\left(p+\frac{a}{v^2}\right) = R(1+at).$$

But how can the factor $(v-b)/v$, which Van der Waals introduces on the left in consequence of the finite diameters of the particles, be justifiably applied to the term in K as well as to that in p? Yet to apply it so is essential to Van der Waals's theory; for without it the resulting equation will not give a cubic in v, and cannot therefore be applied to the isothermals for which it is required. And, in any case, it could scarcely be said that the K term, after being manipulated in this manner, is, in any strict sense, "extracted from the term $\Sigma(Rr)$."

A very strange thing appears, in this connection, in the German version. A result, due it seems to Lorentz (which, in ignorance of his work, I had reproduced and published in the first part of my paper), leads directly to the equation

$$pv = R(1+at)\left(1+\frac{b}{v}\right);$$

which is then put in the confessedly approximate form

$$p(v-b) = R(1+at).$$

Of this it is remarked:—"was genau mit dem obigen Resultate [that obtained by the use of the factor $(v-b)/v$] übereinstimmt." It is obvious that, when we have to divide both sides by $v-b$, we ought to restore the proper factor on the right; and thus that the equation ought to take the final form

$$p+\frac{a}{v^2} = R(1+at)\,\frac{v+b}{v^2},$$

instead of the more convenient form

$$p+\frac{a}{v^2} = \frac{R(1+at)}{v-b},$$

in which Van der Waals employs it. But then it would not give the required cubic in v!

I think that the mere fact of Van der Waals's saying (in a passage which is evidently applicable to his own

---

[1] We have to thank the *Electrician* and the *Electrical Review* for some of the illustrations used in this article.

© 1891 Nature Publishing Group

processes, though it is applied only to that of Lorentz) "die ganze Rechnung doch nur bis auf Grössen der ersten Ordnung (wie b/v) genau ist" throws very grave doubt on the whole investigation. For in the most interesting part of the critical isothermal of CO_2 the fraction b/v cannot be looked upon as a small quantity of the first order. In fact, without raising the question, either of Van der Waals's mode of interpreting the term $\frac{1}{2}\Sigma(mV^2)$ or of the paucity of constants in his equation, the above consideration would of itself render the results untrustworthy. Van der Waals has most opportunely and effectively called attention to an exceedingly promising mode of attacking a very difficult problem, and his methods are both ingenious and suggestive; but I do not think that his results can be regarded, even from the most favourable point of view, as more than "*Guesses at Truth*."

For, if we take the experimental test, there can be no doubt that (as I have stated in § 65 of my paper) "Van der Waals's curves cannot be made to coincide with those of Andrews." And I think I have given reasons for believing that "the term of Van der Waals's equation, which he took to represent Laplace's K, is not the statical pressure due to molecular forces but (approximately) its excess over the repulsion due to the speed of the particles." Of course I mean by this that, when Van der Waals, comparing his equation with experiment, assigns a numerical value to his term a/v^2, he is not justified in regarding it as the value of Laplace's K; though that quantity was, he tells us, the main object of his inquiry.

Believe me yours very truly,

P. G. TAIT.

St. Andrews, September 28.

THE EXISTING SCHOOLS OF SCIENCE AND ART.

AT a meeting of influential science and art teachers held at the Charterhouse School of Science and Art, Goswell Road, on the 3rd instant, the position of existing schools, with regard to the fierce opposition offered by highly-endowed Polytechnics, was calmly and broadly discussed.

For many years, under the system not only recognized but encouraged by the Science and Art Department, schools have been established in London and the provinces. The aid afforded by the Department has mainly been (1) to contribute largely to the building fund of schools intended for the exclusive teaching of science and art subjects, and (2) to remunerate by Government grant the services of the teachers engaged. The regulations of the Department provide that such aid is given to any centre where the need of it is apparent. It is, however, perfectly well known that the teacher, in the majority of cases, was the person upon whom the duty fell to organize the classes and set the ball rolling, and it would be difficult to mention any school or institute in which the motive spirit was not a teacher.

By recent Acts of Parliament a great impetus has been given to that side of science and art instruction known as technical education. Funds which in past times could only have been raised by persistent begging are now forthcoming almost as a matter of routine. In the provinces there is every sign that the authorities having the administration of the grant of public moneys intend to recognize existing schools. In London it is not so. Schemes for the erection of new buildings are pushed forward without due regard to those institutions already doing a good work. At the meeting of teachers already referred to several instances were cited. The People's Palace, erected almost in the very shadow of the Bow and Bromley Institute, has, by reason of its endowment, greatly hampered and harassed the older institution. The West London School of Art succumbed two years ago to the attack of the Regent Street Polytechnic; and now the St. Martin's School of Art, one of the best known centres of instruction in the metropolis, has closed its doors. Without endowment it could not compete with its more favoured rivals. The closing of this school is the more to be regretted because of the high tone of the work carried on within its walls.

Unfortunately, it cannot be denied that many so-called schools of science and art are simply carried on as "grant-earning" establishments, and the country would lose little or nothing if they were closed at once. But there are others affording excellent science and art instruction; and though these may not be affected by the present Polytechnics, it is evident that the schemes yet in embryo for the erection of other buildings will, if not properly checked, raise an undignified competition with the older schools. It is therefore a matter of great public importance that the established institutions should not be overlooked by the London County Council. If new buildings are deemed to be necessary, the old school of science and art should be treated as the nucleus of the enlarged scheme.

Two points of error seem to be apparent in the plan of campaign of the supporters of Polytechnics—(1) that educational work must be associated with recreation; and (2) that technical education has a very limited area, and that science and art education in its fullest sense is unnecessary.

"Schools of art," said a gentleman to me recently, "are dead." Surely nothing could be more absurd. As I understand technical education, it is the application of general principles to a specific purpose. Schools of science and art—*i.e.* schools for the study of science as science, and art as art—should be encouraged as much as before. This can be done without interfering with the specific application of such study to a particular purpose.

With regard to the question of recreation, I think it would be found that, although those institutes which make much of athleticism and such matters attract the largest proportion of students, the attendance *pro ratâ* in the classrooms, and the results obtained there, would not favourably compare with an institute carrying out a purely educational programme. At the meeting referred to, one teacher stated that, although at a Polytechnic with which he had been connected only seven students entered the class, scores of young men could be found in the billiard-room and gymnasium. At the Science and Art Institute, Wolverton, one of the best and most practical schools in the country, it was decided to close the billiard-room in consequence of the serious effect it had upon the attendance of students at the classes. I am personally acquainted with the science and art work carried on at the Regent Street Polytechnic. Excellent as it is, it would be still better if it could be relieved of the recreative element.

The London County Council has shelved for a time the appropriation of the funds provided by the Excise Act, 1890, for the promotion of technical education. But the matter must soon come up again. Healthy competition is excellent, but in this matter it is clearly not to the interest of the public that its money should be used for pushing on a new venture as a competitor to, and in antagonism with, an existing institution. The best butcher's shop in London would stand a poor chance if a rival establishment run with money raised by taxation, and not of necessity expected to pay its way, opened its doors on the opposite side of the road; and this is practically the state of affairs. The teachers, moreover, have a perfect right to be heard on this question. Devoting their best years to the training necessary for science and art teaching, it may be urged that they have a moral, if not a legal, claim to be considered.

In concluding, I would point out that the exponents of technical instruction are too keen on "centralization."

© 1891 Nature Publishing Group